PROCEEDINGS

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# ROYAL SOCIETY OF EDINBURGH.

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*Monday, January* 21, 1850.

Dr CHRISTISON, V.P., in the Chair.

The following Communications were read:—

## 1. On the Gamboge Tree of Siam. By Dr Christison.

Although Gamboge has been known in European commerce for nearly two centuries and a half, and its applications in the arts have been extended in recent times, the tree which produces it is still unknown to botanists.

The late Dr Graham, in 1836, was the first to describe accurately a species of Garcinia, which inhabits Ceylon, and which is well known there to produce a sort of Gamboge, not, however, known in the commerce of Europe. Resting on a peculiarity in the structure of the anthers, which are circumscissile, or open transversely by the separation of a lid on the summit, he constituted a new genus for this plant, and called it *Hebradendron cambogioides*. At the same period the Author examined the properties of this Gamboge, and found that it possesses the purgative action of the commercial drug in full intensity, and that the two kinds agree closely also, though not absolutely, in chemical constitution.

At an earlier period Dr Roxburgh described, in his "Flora Indica," another species of Garcinia, under the name of *Garcinia pictoria*, which inhabits the hills of Western Mysore, and which also

was thought to produce a sort of Gamboge of inferior quality. In 1847 specimens of the tree and its exudation were obtained near Nuggur on the ghauts of Mysore by Dr Hugh Cleghorn of the East India Company's service; and the author, on examining the Gamboge, found it all but identical with that of Ceylon in physiological action, in properties as a pigment, and in chemical constitution. The same plant, with its Gamboge, was about the same time observed by the Rev. F. Mason, near Mergui in Tavoy, one of the ceded Burmese provinces.

A third species, inhabiting the province of Tavoy, and also producing a kind of Gamboge, was identified by Dr Wight in 1840 with Dr Wallich's *Garcinia elliptica*, from Sylhet, on the north-east frontier of Bengal. Its exudation was long thought to be of low quality. But, although this substance has not yet been examined chemically, it has been stated by Mr Mason to be, in his opinion, quite undistinguishable as a pigment from Siam Gamboge.

It is a matter of doubt whether Graham's character is sufficiently diagnostic to be a good generic distinction. But it was shewn by Dr Wight in 1840, that a well characterised section at least of the genus Garcinia consists of species which have "sessile anthers, flattened above, circumscissile, and one-celled;" and that all these species, and no others, appear to exude a gum-resin differing probably very little from commercial Gamboge.

Still the tree which produces Siam Gamboge, the finest and only commercial kind, continues unknown. A strong presumption however arose, that the last species was the Siam tree, as it grows in the same latitude with the Gamboge district of Siam, and not above 200 miles farther west. But if the information recently communicated to the author be correct, the Siam tree is a fourth distinct species of the same section. In December last he received from Mr Robert Little, surgeon at Singapore, specimens taken from two trees which were cultivated there by Dr Almeida, a resident of the colony, and which were obtained by him "direct from Siam" as the Gamboge tree of that country. These specimens are not such as to allow of a complete description; yet they are sufficient to shew that the plant presents the characters of Wight's Gamboge-bearing section of the genus Garcinia; but that it is not any of the species hitherto so fully described as to admit of comparison with it. The fruit is round, not grooved, crowned by a four-lobed knotty stigma,

and surrounded hy numerous sessile or subsessile aborted anthers, and by a persistent calyx of four ventricose fleshy sepals. The male flowers consist of a calyx of the same structure, a corolla of four ventricose fleshy petals, and a club-shaped mass of about forty subsessile anthers, closely appressed, connected only at the mere base, one-celled, flattened at the top, and opening by a circular lid along a line of lateral depressions; and there is no appearance of an aborted ovary amidst them. These are the characters of the three species presently known. These three species very closely resemble one another in general appearance and special characters. The new species presents the same close resemblance to them all; and, in particular, its foliage is undistinguishable from that of *Garcinia elliptica,* the leaves being elliptic, acuminate, and leathery, exactly as described and delineated by Wight. But it differs from them all in the male flowers and fruit being peduncled. The male flowers are fascicled, and have a slender peduncle three-tenths of an inch in length. The single young fruit attached to one of the specimens has a thick fleshy peduncle, like an elongated receptacle, half as long as the male peduncle. All the other species hitherto described have both male and female flowers sessile or subsessile. As this difference cannot arise from a mere variation in the same species, the plant must be a new one. The evidence however that it produces Gamboge, and more especially the commercial Gamboge of Siam, is not yet complete; and, until further information on this point be obtained, which the author expects to receive in the course of the year, it appears advisable not to attach to it a specific name. A question may even arise whether the male flowers and the fruit here described may not belong to two species instead of one; but this is far from probable.

## 2. Notice respecting a Deposit of Shells near Borrowstounness. By Charles Maclaren, Esq.

This deposit of shells is situated about a mile and a half west from Borrowstounness, where the Carse of Falkirk terminates in a strip of flat land a furlong in breadth. The shells are exposed in two openings, each about 300 feet long, made in the soil to procure limestone for Mr Wilson's iron-works. The bed can be traced in these openings along lines having an aggregate length of 1000 feet. Over all that space the shells form an unbroken stratum of

very uniform depth (nearly three inches), and almost perfectly horizontal. They are covered by a bed of dark-brown sandy clay, from two to three feet thick, and rest on a deposit of the same substance, which closely resembles the mud spread over the present beach. The shells are all of one species, the cockle, or *Cardium edule*, and of various sizes down to the most minute. They are mixed with a portion of the clay which covers them, but lie so compactly, that they present to the eye the appearance of a layer of chalk nodules. Very few of them are fractured, and the two valves are generally united. The openings reach within 12 or 15 yards of the high-water line; but the number of broken shells seen on the beach shews that the bed had once extended farther northward, and that part of it has been cut away by the sea. The bed is at present about the level of high water, or a little above it, while the natural abode of the cockle, according to Mr Broderip, is from the low-water line to a depth of 13 fathoms. The continuity of the bed, its regular level, its remarkable uniformity, its composition confined to a single species, and the state of the shells, which are generally entire, and have the two valves united, shew that they are in their native locality, and prove that they could only have been brought to their present position by an upheaval of the land. This upheaval must have been to the extent at least of 18 feet, which is the difference betwixt high and low water, but very probably it was to the extent of 20, 30, or 40 feet. Inundations of the sea, caused by storms, have been called in to account for such deposits, but in my opinion very inconsiderately. That a sudden and violent movement of the sea should sweep away a bed of shells from its original locality, is intelligible enough; but that, while transporting them over some hundred feet or yards, it should preserve them unbroken, with the valves still united,—that the rushing water, instead of ploughing up the dry land it invaded, should smooth and level an area of more than an acre, then spread out the shells upon it with mathematical regularity, in an uninterrupted stratum of nearly uniform depth,—that, finally, it should cover them with a bed of clay two or three feet thick, and then withdraw;—these seem to me to be effects utterly irreconcileable with the known agency of floods. I would as soon believe that the West India hurricane, instead of levelling the planter's house, transports it *en masse*, with it walls, roof, and furniture all entire, from one end of a field to the other.

3. An Account of some Monstrosities. By the late Dr J. Reid. Communicated by Prof. Goodsir.

4. The Effect of Pressure in Lowering the Freezing-Point of Water experimentally demonstrated. By Professor W. Thomson, Glasgow.

On the 2d of January 1849, a communication, entitled "Theoretical Considerations on the Effect of Pressure in Lowering the Freezing-Point of Water, by James Thomson, Esq., of Glasgow," was laid before the Royal Society, and it has since been published in the *Transactions*, Vol. XVI., Part V. In that paper it was demonstrated that, if the fundamental axiom of Carnot's Theory of the Motive Power of Heat be admitted, it follows, as a rigorous consequence, that the temperature at which ice melts will be lowered by the application of pressure; and the extent of this effect due to a given amount of pressure was deduced by a reasoning analogous to that of Carnot from Regnault's experimental determination of the latent heat, and the pressure of saturated aqueous vapour at various temperatures differing very little from the ordinary freezing-point of water. Reducing to Fahrenheit's scale the final result of the paper, we find

$$t = n \times 0{\cdot}0135 ;$$

where $t$ denotes the depression in the temperature of melting ice produced by the addition of $n$ "atmospheres" (or $n$ times the pressure due to 29·922 inches of mercury), to the ordinary pressure experienced from the atmosphere.

In this very remarkable speculation, an entirely novel physical phenomenon was *predicted* in anticipation of any direct experiments on the subject; and the actual observation of the phenomenon was pointed out as a highly interesting object for experimental research.

To test the phenomenon by experiment without applying excessively great pressure, a very sensitive thermometer would be required, since for ten atmospheres the effect expected is little more than the tenth part of a Fahrenheit degree; and the thermometer employed, if founded on the expansion of a liquid in a glass bulb and tube, must be protected from the pressure of the liquid, which, if acting on it,

would produce a deformation, or at least a compression of the glas that would materially affect the indications. For a thermometer of extreme sensibility, mercury does not appear to be a convenient liquid; since, if a very fine tube be employed, there is some uncertainty in the indications on account of the irregularity of capillary action, due probably to superficial impurities, and observable even when the best mercury that can be prepared is made use of; and again, if a very large bulb be employed, the weight of the mercury causes a deformation which will produce a very marked difference in the position of the head of the column in the tube according to the manner in which the glass is supported, and may therefore affect with uncertainty the indications of the instrument. The former objection does not apply to the use of any fluid which perfectly wets the glass; and the last-mentioned source of uncertainty will be much less for any lighter liquid than mercury, of equal or greater expansibility by heat. Now the coefficient of expansion of sulphuric ether, at 0° C., being, according to *M.* I. Pierre,* ·00151, is eight or nine times that of mercury (which is ·000179, according to Regnault); and its density is about the twentieth part of the density of mercury. Hence a thermometer of much higher sensibility may be constructed with ether than with mercury, without experiencing inconvenience from the circumstances which have been alluded to. An ether thermometer was accordingly constructed by Mr Robert Mansell of Glasgow, for the experiment which I proposed to make. The bulb of this instrument is nearly cylindrical, and is about $3\frac{1}{2}$ inches long and $\frac{3}{8}$th of an inch in diameter. The tube has a cylindrical bore about $6\frac{1}{2}$ inches long: about $5\frac{1}{2}$ inches of the tube are divided into 220 equal parts. The thermometer is entirely enclosed, and hermetically sealed in a glass tube, which is just large enough to admit it freely. On comparing the indications of this instrument with those of a thermometer of Crichton's with an ivory scale, which has divisions, corresponding to degrees Fahrenheit, of about $\frac{1}{25}$th of an inch each; I found that the range of the ether thermometer is about 3° Fahrenheit; and that there are about 212 divisions on the tube corresponding to the interval of pressure from 31° to 34°, as nearly as I could discover from such an unsatisfactory standard of reference. This gives $\frac{1}{71}$ of a degree

* See Dixon on Heat, p. 72.

for the mean value of a division. From a rough calibration of the tube which was made, I am convinced that the values of the divisions at no part of the tube differ by more than $\frac{1}{30}$th of this amount, from the true mean value; and, taking into account all the sources of uncertainty, I think it probable that each of the divisions on the tube of the ether thermometer corresponds to something between $\frac{1}{68}$ and $\frac{1}{75}$ of a degree Fahrenheit.

With this thermometer in its glass envelope, and with a strong glass cylinder (Œrsted's apparatus for the compression of water), an experiment was made in the following manner:—

The compression vessel was partly filled with pieces of clean ice, and water: a glass tube about a foot long and $\frac{1}{10}$th of an inch internal diameter, closed at one end, was inserted with its open end downwards, to indicate the fluid pressure by the compression of the air which it contained: and the ether thermometer was let down and allowed to rest with the lower end of its glass envelope pressing on the bottom of the vessel. A lead ring was let down so as to keep free from ice the water in the compression cylinder round that part of the thermometer tube where readings were expected. More ice was added above, so that both above and below the clear space, which was only about two inches deep, the compression cylinder was full of pieces of ice. Water was then poured in by a tube with a stopcock fitted in the neck of the vessel, till the vessel was full up to the piston, after which the stopcock was shut.

After it was observed that the column of ether in the thermometer stood at about 67°, with reference to the divisions on the tube, a pressure of from 12 to 15 atmospheres was applied, by forcing the piston down with the screw. Immediately the column of ether descended very rapidly, and in a very few minutes it was below 61°. The pressure was then suddenly removed, and immediately the column in the thermometer began to rise rapidly. Several times pressure was again suddenly applied, and again suddenly removed, and the effects upon the thermometer were most marked.

The fact that the freezing-point of water is sensibly lowered by a few atmospheres of pressure, was thus established beyond all doubt. After that, I attempted, in a more deliberate experiment, to determine as accurately as my means of observation allowed me to do, the actual extent to which the temperature of freezing is affected by determinate applications of pressure.

In the present communication, I shall merely mention the results obtained, without entering at all upon the details of the experiment.

I found that a pressure of, as nearly as I have been able to estimate it, 8·1 atmospheres produced a depression measured by $7\frac{1}{2}$ divisions of the tube, on the column of ether in the thermometer; and again, a pressure of 16·8 atmospheres produced a thermometric depression of $16\frac{1}{2}$ divisions. Hence the observed lowering of temperature was $\frac{7\frac{1}{2}}{71}$, or ·106° F. in the former case, and $\frac{16\frac{1}{2}}{71}$, or ·232° F. in the latter.

Let us compare these results with theory. According to the conclusions arrived at by my brother in the paper referred to above, the lowering of the freezing-point of water by 8·1 atmospheres of pressure would be $8{\cdot}1 \times {\cdot}0135$, or ·109° F.; and the lowering of the freezing-point by 16·8 atmospheres would be $16{\cdot}8 \times {\cdot}0135$, or ·227° F. Hence, we have the following highly satisfactory comparison, for the two cases, between the experiment and theory.

| Observed Pressures. | Observed Depressions of Temperatures. | Depressions according to Theory, on the hypothesis that the Pressures were truly observed. | Differences. |
|---|---|---|---|
| 8·1 Atmospheres. | ·106° F. | ·109° F. | − ·003° F. |
| 16·8 Atmospheres. | ·232° F. | ·227° F. | + ·005° F. |

It was, I confess, with some surprise, that, after having completed the observations under an impression that they presented great discrepancies from the theoretical expectations, I found the numbers I had noted down indicated in reality an agreement so remarkably close, that I could not but attribute it in some degree to chance, when I reflected on the very rude manner in which the quantitative parts of the experiment (especially the measurement of the pressure, and the evaluation of the division of the ether thermometer) had been conducted.

I hope, before long, to have a thermometer constructed, which shall be at least three times as sensitive as the ether thermometer I have used hitherto; and I expect with it to be able to perceive the effect of increasing or diminishing the pressure by less than an atmosphere, in lowering or elevating the freezing-point of water.

If a convenient *minimum* thermometer could be constructed, the effects of very great pressures might easily be tested by hermetically sealing the thermometer in a strong glass, or in a metal tube, and putting it into a mixture of ice and water, in a strong metal vessel, in which an enormous pressure might be produced by the forcing pump of a Bramah's press.

In conclusion, it may be remarked, that the same theory which pointed out the remarkable effect of pressure on the freezing-point of water, now established by experiment, indicates that a corresponding effect may be expected for all liquids which expand in freezing; that a reverse effect, or an elevation of the freezing-point by an increase of pressure, may be expected for all liquids which contract in freezing; and that the extent of the effect to be expected may, in every case, be deduced from Regnault's observations on vapour (provided that the freezing-point is within the temperature-limits of his observations), if the latent heat of a cubic foot of the liquid, and the alteration of its volume in freezing be known.

## 5. On the Extinction of Light in the Atmosphere. By W. S. Jacob, Esq., H.E.I.C. Astronomer, Madras. Communicated by Prof. C. Piazzi Smyth.

In a letter dated Madras, November 1849, Captain Jacob says, "I have been much interested in reading, lately, Professor Forbes's paper in the Philosophical Transactions, 1842, Part 2, on the Extinction of Light and Heat in the Atmosphere." As his results agree very closely with those of my experience on the Trigonometrical Survey of India, and which, though not founded on any precise measures, being still the conclusions of some years' experience, are perhaps worth noticing, particularly when they agree with the results of more exact measures.

On commencing work with heliotropes in 1837, I soon found that for long distances it was necessary to enlarge the apertures *more* than in the simple ratio of the distance (though such was Colonel Everest's practice); and before the end of the first season, I had formed a scale of apertures for corresponding distances, which afterwards needed very little alteration, but when finally corrected by subsequent years' observation, stood as follows :—

| Aperture. Inches. | Maximum Distance. Miles. | Maximum Distance without Absorption. |
|---|---|---|
| 0·5 | 15 | 15 |
| 1·0 | 23 | 30 |
| 2·0 | 33 | 60 |
| 4·0 | 45 | 120 |
| 8·0 | 60 | 240 |

Our heliotropes were circular glass mirrors, 8 inches in diameter; and for the smaller apertures, diaphragms were used between the heliotropes and the observer. At the distances stated the light was just visible to the naked eye in clear weather, and when seen over a *valley*: if the ray *grazed* near the surface, the light was much reduced. On one occasion I employed a heliotrope at 6¼ miles, and used an aperture of ⅙ of an inch, and found it rather brighter than usual, so that probably 6½ or 7 miles would be the normal distance for that size.

This agrees well enough with the rest of the scale, but there is no need to employ a conjectural quantity; and if the rate of absorption corresponding to the above be computed, so close an agreement will be found, as may entitle the numbers to be looked on as something better than mere estimates,—as the results, indeed, of a species of observation.

The mean of the whole shews a loss of ·0610 in passing through one mile of atmosphere; with the barometer at 27·0 inches (that being about the average height of my stations), but reduced to 30·0 inches, the quantity will be ·0671.

Hence the loss of light in passing from the zenith through a homogeneous atmosphere of 5·2 miles will be ·303, or only about one per cent. less than Professor Forbes's result. And as my air was considerably drier than his (the mean humidity being not much above ·30 instead of ·56), this will probably account for the difference; and, at any rate, the agreement is much closer than could have been expected.

I once mentioned this matter to Captain Waugh, the present Surveyor-General of India, then my fellow-assistant; but he not only had not noticed the thing, but did not even apprehend my meaning. He assented to my remark on the *loss* of light in passing through the atmosphere, but asserted that the aperture should vary as the distance, thus allowing for *no* loss! 0·1 inch per mile answered,

he said, for all distances that he had tried! So it might answer for the distances most usually occurring on the Survey; for 4 inches would be proper for 40 miles, and 2 inches not much too bright at 20, and it is not often that these limits would be passed. Yet it is hardly possible to conceive that he should not have noticed the different intensity of the lights; had not his opportunities been perhaps rather unfavourable, as his work lay chiefly in plains, where, as mentioned above, the light of a grazing ray is very much reduced, and the atmospheric effect would therefore be mixed up with disturbing local causes.

I myself was much astonished at first discovering that the air had so great absorbent powers, and many ideas are suggested by the fact. We see at once how easily many of the planets may be rendered habitable to beings like ourselves. Mars, *e. g.*, may enjoy a temperature little inferior to our own, by having a *less* absorbent envelope; and Venus may be kept as cool as we are, by having one *more* so.

The following Gentlemen were duly elected Ordinary Fellows:—

Mr Alex. K. Johnston.
Dr John Scott, F.R.C.P.
Dr Sheridan Muspratt, Liverpool.

The following Donations to the Library were announced:

Annuaire Magnétique et Météorologique du Corps des Ingénieurs des Mines; ou Recueil d'Observations Météorologiques et Magnétiques faites dans l'entendue de l'empire du Russie, par A. T. Kupffer. Nos. 1 & 2, 1849. 4to.—*By the Russian Government.*

Verhandelingen der Eerste Klasse van het K. Nederlandsche Instituut van Wetenschappen, Letterkunde, en Schoone Kunsten te Amsterdam. 3de Reeks, Deel 1, Stuk 3 en 4. 4to.

Tijdschrift voor de Wis-en Natuurkundige Wetenschappen uitgegeven door de Eerste Klasse van het K. Nederlandsche Instituut van Wetenschappen te Amsterdam. 3de Deel, 1 & 2 Afleverings. 8vo.—*By the Academy.*

Jaarboek van het K. Nederlandsche Instituut van Wetenschappen, Letterkunde, en Schoone Kunsten te Amsterdam, 1847, 1848, 1849. 8vo.—*By the Academy.*

Catalogue of 2156 Stars, formed from the Observations made during Twelve Years, from 1836 to 1847, at the Royal Observatory, Greenwich. 4to.—*By the Royal Society, Lond.*

Proceedings of the Philosophical Society of Glasgow, 1848–9. Vol. III., No. 1. 8vo.—*By the Society.*

Quarterly Journal of the Chemical Society of London. No. 8. 8vo. *By the Society.*

Proceedings of the Royal Astronomical Society. Vol. X., No. 2. 8vo. —*By the Society.*